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CONTENTS

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WHITMAN'S WORK ON THE EVOLUTION OF THE GROUP OF PIGEONS

THE three volumes containing the work of Professor Charles Otis Whitman on pigeons published by the Carnegie Institution of Washington is a fine memorial to one of the leaders of zoological research in America. In the course of the sixteen years devoted to this work Whitman brought together birds from all parts of the world, bred them, studied their juvenile and adult plumages, and their habits, and made many crosses between different species. When he died in 1910, his extensive and valuable collection of living birds was saved through the devotion and sacrifices, both personal and financial, of Dr. Oscar Riddle, the editor of these posthumous volumes. After that first year of precarious existence, the Carnegie Institution met during the five years following the expenses of maintenance, and during this time the birds, under Dr. Riddle's care, were transferred to the laboratory at Cold Spring Harbor where Whitman's work is being carried forward. Without this support only a fragment of Whitman's results could have been preserved or the birds kept to complete many of the important problems that were at the time of Whitman's death still unfinished. The editing of the work has been admirably done by Dr. Riddle. It is a fortunate circumstance that what was left fell into the hands of one familiar with Whitman's ways of thinking, and thoroughly conversant with the many problems that had grown out of Whitman's studies; for "not more than one fifth of the matter" was in shape for publication when Whitman died.

Volume I. gives Whitman's views and his evidence for orthogenetic evolution. The editor says in the preface, Whitman "has accumulated the most weighty evidence for

[1] Posthumous words of Charles Otis Whitman. The Carnegie Institution of Washington, 1919.

continuity as against discontinuity in the phenomena of variation, inheritance and evolution." And with this verdict his reviewer is not inclined to disagree, because as a careful study of Whitman's evidence and meaning shows, there is not much difference between what he understood by continuity and what is to-day called more often discontinuity.

In the introductory chapter from a manuscript written in 1909 that formed part of a lecture given at Clark University, the keynote to Whitman's antagonism to the mutation theory of de Vries is struck—a note that recurs throughout the first two volumes. Weismann, he says, taught us to look to germinal variation as the source of all variation that is hereditary. Then follows a paragraph that takes us to the heart of the matter: "Do we not have, then, in germinal variation, a better criterion of what is specific than we get in sudden appearance? Indeed, is it not here that the seeming suddenness of first appearance finds its explanation, and likewise the fact that so-called mutations involve the whole organism? If we are to accept the physiological conception of development, as is inevitable in my opinion, it is easy to see that a change, however slight, in the primordial constitution of the germ would tend to correlate itself with every part of the whole germ-system, so that the end stage of development would present a new facies and appear as a total modification, answering to what deVries would call a mutation. That some thing of this order does sometimes occur I have indubitable evidence, and in such form as to dispel the idea of discontinuity and sudden gaps in transformation."

With a slight shift of wording and emphasis the essential part of this statement is not very different from what we think to-day, for who will dispute now that a change (mutation) in the germ-plasm may affect many parts of the organism that develops out of such a changed germ-plasm? Such a view has not been found to dispel the idea of "discontinuity" of characters; on the contrary it is in full accord with it.

But the unit character is Whitman's *bête noir*. "The idea of unit-characters, however, as distinct elements that can be removed or introduced bodily into the germ does not appeal to me as removing difficulties, but rather as hiding them; in short, as a return to the old pangenesis view of preformed characters. In this theory, as is well known, we have two miracles involved. The first consisted in a centripetal migration of preformed gemmules, and the second in the centrifugal distribution of the same elements. DeVries dismisses the first of these, but accepts the second, and on it rears the superstructure of his theory of mutable-immutable unit-characters. With all due respect to the distinguished author of this theory, and with abounding admiration for his great work and model methods, which have aroused universal interest and stimulated enormously experimental bionomics, I am strongly persuaded that his hypothesis of unit-character fails as a guide to the interpretation of the species and its characters."

"It is true a great amount of work on Mendelian heredity seems strongly to support the unit-character hypothesis, and that cytology offers some further support. Nevertheless, I have to confess a wholesale scepticism. The germ, as I believe and have long maintained, stands for an organized whole. It is a unit-organism, not an organism of units; all the features that arise in the course of development are within the sphere of the individual unity and integral parts of it, and whatever specificity they possess is completely determined and not of independent origin."

"The strongest suggestion of unit-characters is found in the phenomenon known as segregation. I do not understand the importance of this striking behavior of so-called alternative unit-characters. I am familiar with it and deeply interested; but I am unable to see in them the sum total of all we know about heredity. What I have said in regard to unit-character applies to the Mendelian doctrine. Mendelism, like mutation, neglects the natural history of the characters, it experiments with and is not primarily concerned to know how characters have originated and multiplied."

It may be that the emphasis laid on unit-character by some of the earlier enthusiastic followers of Mendel and the frequent confusion in their writings between the unit-character, so-called, and the change in the germ-plasm that gave rise to it, may justify Whitman's scepticism; but this charge can hardly be brought against de Vries, who stated over and over again that a single change in the germ-plasm may be the cause of manifold although slight changes in the characters throughout the whole organism.

In contrast to change by mutation Whitman opposes orthogenesis. Evidence for the latter he finds in his study of the group of pigeons. The evidence is the familiar argument from comparative anatomy and from the hypothesis of "recapitulation."[2] Before taking up the evidence I can not refrain from quoting a fine and characteristic statement of Whitman's in the same lecture:

"I take exception here only to the implication that a definite variation-tendency must be considered teleological because it is not 'orderless.' I venture to assert that variation is sometimes orderly and at other times rather disorderly, and that the one is just as free from teleology as the other. In our aversion to the old teleology, so effectually banished from science by Darwin, we should not forget that the world is full of order, the organic no less than the inorganic. Indeed what is the whole development of an organism if not strictly and marvelously orderly? Is not every stage, from the primordial germ onward, and the whole sequence of stages, rigidly orthogenetic? If variations are deviations in the directions of the developmental processes what wonder is there if in some directions there is less resistance to variation than in others? What wonder if the organism is so balanced as to permit both unifarious and multifarious variations? If a developmental process may run on throughout life (*e. g.*, the lifelong multiplication of the surface-pores of the lateral-line system in *Amia*) what wonder if we find a whole species gravitating slowly in one or a few directions? And if we find large groups of species all affected by a like variation, moving in the same direction, are we compelled to regard such 'a definite variation-tendency' as teleological, and hence out of the pale of science? If a designer sets limits to variation in order to reach a definite end, the direction of events is teleological; but if organization and the laws of development exclude some lines of variation and favor others there is certainly nothing supernatural in this, and nothing which is incompatible with natural selection. Natural selection may enter at any stage of orthogenetic variation, preserve and modify in various directions the results over which it may have had no previous control."

How far one is justified in extending the orderly sequence of embryonic development to the sequence shown in evolutionary advance is a large question and will no doubt be settled some day by fuller knowledge. At present our speculations must rest on the evidence at hand, and this evidence, Whitman finds, as stated, in his comparative studies of pigeon coloration, and in a most ingenious experiment of feather plucking.

His studies of domesticated breeds and their wild relatives led him to conclude that the blue wing with two black bars is not the original pattern as Darwin supposes, but rather the checkered wing covered with black spots. Both patterns are found to-day in wild birds, hence these birds can not be appealed to for a decision. But an examination of other species of pigeons shows that the checkered type is widespread and occurs in many varieties; and the young in many groups show a more checkered pattern than do the adults themselves. The Japanese turtle dove comes nearest, in Whitman's opinion, to the original type of wing pattern. The elaborate consideration that Whitman devotes to the subject indicates how important the question appeared to him;

[2] Whitman uses the word "recapitulation" in the sense for which the reviewer argued in 1903 ("Evolution and Adaptation," Chap. III.). As so used it means something essentially different from the word "recapitulation" in the original sense of Darwin and Haeckel, unless the changes in the germ-plasm add stages only to the end of ontogeny as Whitman seems to think is the way in which the process takes place. (See a later footnote.)

for, from it he derives the support of his orthogenesis. Since the same kinds of advances are observed over and over again in different groups, and since no plausible reason can be given why such changes are of benefit to the species, it follows, on Whitman's view, that some internal agency has brought about these parallel advances.

The change at molting that transforms the young plumage into that of the adult is often abrupt, almost like a mutation, yet a simple experiment shows that in the interval the constitution of the bird has been progressively advancing. If feathers are plucked in the intervening stages, the new feathers show an advance over the young feathers still present, an advance in the direction of the feathers that are to come at the next molt. And the nearer to molting time the operation is performed the nearer the approach to the newer feathers. Here then what appears to be a sudden change has in reality been led up to by a continuous series of preparatory stages; so, in Whitman's view, what appear at times to be sudden and great changes in evolution (mutations) are in reality only end stages of continuous advance. The development of the bird repeating the history of the race shows continuous change but the exegesis of molting gives us only the earlier and the later picture. To discuss this theme would take us too far afield, but it is a matter not unfamiliar to the morphologist. It should be pointed out that a change (mutation) in the germ-plasm affecting principally the end stages would be expected to give results that are in no sense incompatible with this picture.

Whitman obtained a few "mutations," *i. e.*, new types of pattern that were transmitted. The mutant change, he points out, is only an extension of a character already faintly present in the birds and present in certain wild species. What is produced is not new but a "continuous" extension of a character already present. Hence such mutations are not, he contends, new unit-characters but extension or diminution of characters already in existence. Such, in fact, are the majority of mutations known to us to-day.

Whitman thinks a very old idea reincarnated in Darwin's theory of pangenesis (that the body characters impress their influence on the germ cells) while nominally rejected survives in more subtle guise in some more modern theories such as de Vries's theory of pangenesis. In this theory the nucleus is looked upon as the seat of the hereditary complex. Its "vital" units are self-perpetuating by division, so that the nucleus in every cell remains the store house of all of the hereditary materials. In the course of embryonic development these vital elements, pangenes or genes, are set free in the surrounding cytoplasm of the cell, where they multiply and determine the fate of the cell. "The myth of transmission was not eliminated; it was only reduced in its field." "Transmission thus became more direct, but its mysteries remained as unfathomable as before. The unit-characters are assumed to preëxist in the chromosomes and to stand in need of transportation from the nucleus to the body of the cell in order to develop." But "if an innumerable host of specifically distinct unit-characters are let loose in the cell-plasm, how are they to reach precisely predetermined points in the organism, and at just the time when needed? It is here that the theory breaks down, for the difficulty is not one that further investigation may hope to solve, but one that lands us in hopeless speculation. So long as the primary assumption is that of ready-made unit-characters, specifically distinct and independently variable, whether located in the nucleus or in the cytoplasm, or in both, the problem of development will remain inscrutable."

A perusal of de Vries's pangenesis theory will show that Whitman has put his finger on a weak spot in the speculation, in so far as this view pretends to explain how the specific pangens of the nucleus are supposed to migrate out of the nucleus of each cell at the right time in particular regions of the embryo, but de Vries laid no emphasis on this and was familiar with the absence of evidence for such an interpretation. The same difficulty confronts us to-day, but if I understand

the situation rightly no one would be bold enough to claim any such time relations of pangen migration nor does the theory of nuclear influence call for such a hypothesis in any sense. It is ony necessary that nuclear influence should in some way affect the chemical changes that go on in the surrounding cell to cover completely the situation. No time relation is expected or called for, and who to-day will deny, in the face of extensive evidence, that the nucleus does have an important influence on the cell? With this understanding one can agree cordially with Whitman's concluding thrust: "The doctrine of germs laden with independent unit-characters, or pangens, each predestined, so to speak, to flower in its own place and time strikes me as teleological mythology, fine spun, to the verge of absurdity. We have not yet fathomed primordial organization, but it is safe to assume that the germ sets out with a biophysical constitution of a given specific type, within which metabolic, generative and differentiating processes under normal conditions run on in a self-regulating way."

The title of Volume II. epitomizes its contents, "Inheritance, Fertility and the Dominance of Sex and Color in Hybrids of Wild Species of Pigeons." Seven manuscripts of less than one hundred pages, nearly 2,000 pages of breeding records, and two hundred illustrations comprised the original material of this volume of two hundred twenty-three pages. Only a few chapters, viz., I. (1904–05), XII. (1897), XVI. (1898), and XVII. (1906) were left complete. The remaining chapters (containing fragments and sections by Whitman, and his breeding records) consist in large part of analyses and discussions by the editor based on Whitman's data to which have been added many of the later observations and views of the editor. This work of elucidation and summarization has been well done, making the text readable, and guiding the reader through a maze of not completed and intricate data.

One of the outstanding results of the hybridization work, which constitutes the bulk of this volume, is that offspring produced by crossing species of generic or family rank are males. This fact is in conformity with results obtained in other species of birds (see Guyer). The result is however complicated, according to the editor, by a second result, viz., "that, in many crosses of very distinct genera and species, fertility (developmental power) is shown to be highest in the spring and lowest in the autumn, and that male offspring predominate in the season of highest fertility, while females largely predominate in the season of lowest fertility." Several pages attempting to explain the apparent contradiction follow this statement, but since "it may be emphasized that Professor Whitman was by no means inclined to dogmatize as to the interpretation of this sex series," the subject need not be further discussed here.

In certain crosses between checkered and barred domesticated races the results show that checkered birds may throw some barred offspring. That the two may differ by a single factor difference may seem probable, especially in the light of other evidence (Bonhote and Smally, Staples-Browne) not referred to in the text. The relation is mentioned here because it elucidates a point not fully understood by opponents of Mendelian interpretation, viz., that such a relation is not claimed by most Mendelians as showing necessarily that the barred character must have arisen by a single mutation, although it may have done so. There may have been, as Whitman thinks, a long line of more graded intermediate steps between the two; still the barred and the checkered types might be differentiated to-day by a single factor difference provided both contained all other genes in common. In other words the modern checkered and barred birds, as compared with the old checkered type, would be supposed to carry an entire series of gradually acquired factors, and the checkered birds one further factor. Thus one change in the complex that gave the barred type is supposed to have sufficed to suppress all of the new stages. The two checkered birds would differ then in the entire series of gradually acquired factors, and also in the single final factor that caused the apparent back-throw. There are also records, some of them too fragmentary to be

significant, bearing on the question of the greater likelihood of the first egg being a male in "pure" species—a question that goes back to Aristotle and has as often been denied as affirmed. A table on page 171 (Table 170) appears to indicate that this is the case in the Streptopelia senegalensis where twelve males came from the first egg, and only two females came from the first egg, while only two males came from the second egg and nine females from the second egg. The evidence that has been advanced in refutation of this relation is due, the editor suggests, to the use of "mongrels, collectively known as domesticated pigeons." More data must be obtained and statistical treatment applied to settle this question. The genetic evidence shows that the female is heterozygous for the sex-chromosome, and if the method of disjunction of the sex-chromosome in the egg is affected by the conditions that prevail when the first egg is set free from the ovary, we may possibly find in this relation an *excuse* for such a result. If this should turn out to be true, the cause of the maleness of the generic hybrids must be sought in some other direction.

The chapter (XIV.) on Heredity contains mainly the more general points of view reached by Whitman in 1907. Coming at a time when Mendel's discoveries had received general notice and had been, even then, confirmed from many sources, the chapter contains results of exceptional interest. The grounds for Whitman's objection to any theory resting on the assumption of unit-characters is contained in the following striking paragraph:

"Every theory founded upon the postulate of unit-characters, or specific determinants stored in the nucleus is necessarily committed to some form of centrifugal distribution during the course of development; and for each element to be distributed it is necessary to assume either that it is passively transported to its destination or that it finds its own way automatically. In either case it would be nothing less than a miracle for a specific pangen to reach a prescribed point in such a complex mosaic field as the organism represents; and, for this to be fulfilled, not only at the predetermined point, but also just at the moment for harmonious development with its immediate neighbors, with symmetrical and correlated groups, with inter- and intra-locking systems constituting a microcosmic whole, incomparably more difficult to grasp than the stellar universe—for all this to be fulfilled is utterly beyond the bounds of scientific credibility. To try to conceive of normal development as thus prepunctuated in all its time and space relations—as proceeding from ready-made elemental characters, automatically distributing themselves or guided by entelechies —is to indulge in ultra-scientific teleology."

The statement imputes apparently, to Mendelism in so far as it deals with unit-factors and unit-characters an implication from de Vries's hypothesis of pangenesis; viz., the migration from the nucleus of "organic bodies" which multiply in the cytoplasm and determine the fate of the cell. There is the further implication that the migration is so timed that it takes place at each critical place in development. With Whitman's criticism most students of heredity will agree, but it should be noted, as I have pointed out above, first that Mendelism makes no such appeal, second that the relation of specific materials in the nucleus need not be supposed to have any such time relations as here stated, and third a careful reading of de Vries's "pangenesis" shows that he does little more than make a passing reference to such an interpretation and to-day, at any rate, it is not an essential part of the doctrine of nuclear action. Whitman's own view makes it evident that he is not inclined to disregard the nucleus as one of the elements in the "organization" that supposedly has some action on "the cell as a unit." Granting that differences may exist in the nucleus of different species, different end products are expected. The evidence that such differences may be related to specific substances in the nucleus is no longer a speculation but rests on the analytical evidence from Mendelian heredity. In what way and at what times the nuclear materials take part in the determination of characters we do not know. The essential point is that we are in

no way committed to any interpretation. Stated negatively we might add that there is nothing known at present to preclude the possibility that the influence is a purely chemical process. We find ourselves, therefore, practically in agreement with Whitman's attitude when he says:

"Now while ontogeny is so wonderfully exact that we never cease to be amazed at its performances, we must not forget that germ-cells are subject to slow variation. In fact, it is only germ-variation that has to be considered in phylogeny as in ontogeny. Consequently, when the germ-cell takes a step forward, ontogeny begins with an initial difference that sets the whole series of ontogenetic stages on a diverging line that digresses so little as to be undiscoverable until nearly at the end of development."[3]

Whitman's failure to find "dominance and recessiveness" of character in his pigeon crosses led him to attack the supposed importance of these relations. To-day we know more cases where the hybrid shows in some degree an intermediate development of the contrasted characters than where dominance is complete Obviously the distinction has no importance since the law of segregation is found to hold as well when blending occurs as in cases where the somatic differences are clearly evident. The hybrid pigeons fall, therefore, in this respect into line with familiar phenomena. The failure of "splitting" in subsequent generations is a point that calls to-day for special consideration, but will not be dwelt on here.

In this chapter, and in several that precede it, Whitman and the editor speak rather frequently of what is called "weak" and "strong" germs as having an importance in determining the "strength" to which a character develops, even causing a "reversal of dominance." Curiously enough their effects are supposed to be transmitted so that fertility in the offspring is also affected. Even the occasional mutations found by Whitman are ascribed to this source. Pigeons unquestionably furnish unusual material for the study of this appearance. It is perhaps too soon to attempt to state how much or how little in variation to ascribe to such an influence, aside from the obvious effect in the immediate offspring. No doubt further work along these lines will help us to define more sharply what is to be understood by the somewhat vague attributes "weakness" and "strength."

There are important discoveries recorded in this volume that can only be referred to briefly; the "divisibility" of characters (meaning intermediate conditions) as seen in hybrids, the study of a "dominant" mutant character; the discovery as early as 1896 of sex-linked inheritance (of which a number of cases in other birds are well understood to-day), the cross between the last surviving members of our wild passenger pigeon and the ring dove, the relative influence of egg and sperm on the time of hatching of the hybrid young. Each of these results marks an advance in our understanding of heredity.

The third volume containing Whitman's observations on the "Behavior of Pigeons" is edited by Professor Harvey A. Carr. Thirty-two short manuscripts were left. It appears that Whitman's first period of study in this field was from 1895–98. In a few lectures at Woods Hole in 1897–98 some of his conclusions are given. After a period of five years a renewed interest in these directions recurred and many notes were made. The Woods Hole lecture in 1906 gave an opportunity for further consideration. Despite the very fragmentary remains of this work—fragmentary only in comparison with the extensive observations that Whitman had made, this volume contains many observations of great interest and gives an insight into the character of Whitman's methods, where the most careful and minute observations are interpreted with a breadth of intelligence that is truly remarkable. There are few if any groups of animals

[3] The reviewer would add an important reservation, viz., that a "forward step" in the germ-plasm might affect any stage in the course of development, or in the extreme case every stage in the development. This view is obviously consistent with what Whitman states, but, if emphasized, would to a large extent undermine the value of the evidence from ontogeny in interpreting ancestral stages.

so well suited to studies of this kind as are the pigeons. The elaborate courtship, the fidelity of the individuals to each other, the mating and nesting habits, the part taken by the female and the male in incubation, the feeding instinct of old and young, the weaning and the rythmic sequence of broods offer a fascinating opportunity to the student of animal behavior. Whitman obviously had in view a large program toward the accomplishment of which he had progressed much further that these notes indicate. Some of the lines of work opened up by him have been pursued successfully by his students Professor Craig and Dr. Riddle, but according to their statement his knowledge far outstripped that of any other observer in this field. The many observations here recorded are clearly only the material out of which, in time, he had expected to link up the evolution of instincts with the study of the evolution of structure and color. "If Professor Whitman had completed his work, he would have produced an extensive treatise on the phylogney of the pigeon group. . . . The voices and the behavior of the various species would have been used, like the color patterns, to throw light on the relationships, derivation and method of origin of pigeon species" (Craig and Riddle). According to Carr, Whitman developed "what one may term an orthogenetic conception of instinctive development. Instincts are not novel and unique constructions which spring, without ancestry, into being; rather each new instinct is but a slight modification or organization of tendencies already in existence." When one sees how vital the instincts are for the existence of the species it is probable that however the changes originated the advances would most probably be those involving only slight modifications of intincts already in action.

The Carnegie Institution and equally Dr. Riddle are to be sincerely congratulated on having preserved for American zoologists the last great work of Whitman. The wonderful colored pictures, almost entirely the work of the Japanese artist Hyashi, are marvels of beauty and accuracy, and stand for the minute attention that Whitman demanded at every stage of his work. The same attention to detail is shown in Whitman's early work on cell-lineage, on the leeches of Japan, and on the embryology of fishes, and explains in part his far reaching influence on American zoologists. It is rare to find combined such delicacy in treatment of detail with the sweep of philosophical interpretation of which Whitman was equally master.

Whitman stood at the parting of the ways. We may regret that he did not enter into the new era that even at that time was opening up its far reaching vistas, but this need not blind us to the fine example he set—an example of unworldly devotion and absorption in his work, of self-criticism made possible by simplicity and honesty of character, of fairness that led him to appreciate and to state accurately and kindly the opinions of others with whom he disagreed heartily.

T. H. Morgan

Columbia University

A PALEONTOLOGIC REVIVAL AT YALE UNIVERSITY

Othniel Charles Marsh was appointed professor of paleontology at Yale in 1866, this being the first time such a chair was established at any university He was unquestionably one of America's leading men of science, and in vertebrate paleontology "he stood without a peer." He had collected fossils long before his graduation from Yale in 1860, and after taking the doctorate at Heidelberg, he became deeply interested in the wonderful array of extinct vertebrates that the U. S. Geological and Geographical Survey of the Territories was finding in the "bad lands" of Nebraska. In the meantime, his uncle, George Peabody, had founded at Yale the Peabody Museum of Natural History, though the building was not erected until 1875. Marsh saw the great western wilderness for the first time in 1868, going over the Union Pacific into Nebraska and Wyoming. In 1870 he fitted out the first Yale College Scientific Expedition, and took west with him twelve enthusiastic students. From this time the flood of boxes shipped to the university grew annually greater and greater. In 1899 Pro-

fessor Beecher said of these collections: Professor Marsh "brought forth in such rapid succession so many astonishing things that the unexpected became the rule. The science of vertebrate paleontology could not assimilate new material so fast. . . . The constant stream of vertebrate riches which, from 1868 to 1899, flowed into the Peabody Museum from the Rocky Mountain region had a similar bewildering effect upon Marsh, for it was impossible for him to do more than seize upon what appealed to him as the most salient. As a collector Marsh was seen at his best, and the collections he amassed during his forty-five years and more of activity in this direction form a lasting monument to his perseverance and foresight."

In Marsh's day, Peabody Museum was a very busy place, with a large staff unearthing and preparing the collections so that the master mind might make the treasures known to science. At least 400 new species and 185 new genera were described in abbreviated form previous to 1896, mainly in the *American Journal of Science.* In 1892 came the first check to his activity, and Marsh had to let go a considerable portion of his staff. He was then sixty-one years of age, but he struggled on, thinking that somehow he could describe the great mass of still unknown animals assembled in the museum, and make them fully known in large monographs Seven years later the Great Reaper took him, with his work still undone.

Professor Charles E. Beecher took up the work after Marsh's death, but he had no one to assist him in unearthing the collections except two preparators. Even under these conditions, however, the public were shown for the first time the skeletons of some of the wonderful animals of the past mounted as they appeared in life. The exhibition collections grew apace, and long before Professor Schuchert succeeded Beecher in 1904, they had outgrown the building. Two years later Professor Lull was added to the staff. Now we have mounted or ready to mount so many of our treasures that we are yearning for the new Peabody Museum, to take the place of the original building which was destroyed in 1917 to make way for the Harkness dormitories.

Professor Marsh left $30,000 "to be expended by the trustees of said Peabody Museum in preparing for publication and publishing the results of my explorations in the West." The trustees have heretofore held that only the income of this fund should be used in this way. However, having only this income to devote to the Marsh Collections, it was but natural that progress should be slow. We have now come to realize this fully, and the recognition has brought use to a new turn in the administration of the collections.

As it was evidently Professor Marsh's wish that both the income and the principal of the "Marsh Publication Fund" should be used in work on his collections, the trustees of the museum have recently decided to spend as much of the fund as will be required to make known the collections. The study of the Marsh material is therefore progressing far more rapidly than it has at any time since the donor's death. We have now on the staff of the museum, working under the guidance of Professor Lull, besides the two preparators, the following research associates: Dr. George F. Eaton and Assistant Professor John P. Buwalda, who give us half their time, and Drs. Edward L. Troxell and Malcolm R. Thorpe, who devote all their time to the Marsh collections.

In unearthing the unknown in science, no one can predict what the results will be, or how quickly they will be forthcoming, but we trust that in this case they will be abundant and timely. In working out the new things, however, we have also to consider the old ones, which, viewed in the light of the knowledge of to-day, were inadequately described. How vast are the treasures that Professor Marsh has left us is not even at this time fully known to the curators, but if it should take from ten to twenty years more to complete the description of the fossil vertebrate material assembled by Professor Marsh, Yale will but be the richer scientifically.

CHARLES SCHUCHERT

YALE UNIVERSITY

WILLIAM GILSON FARLOW[1]

The Botanical Society of America records its appreciation of the great loss sustained by the society, by American science, and by botanical science throughout the world, in the death of Professor William Gilson Farlow.

Educated as a physician, he yielded readily to Asa Gray's suggestion that he broaden the scope of botany at Harvard University by developing there an interest in flowerless plants, which up to that time had scarcely appeared above the horizon of professional botanists in America. In preparation for this he traveled extensively in northern Europe, at a time when extended travel was uncommon, meeting and forming personal relations with the leading authorities on cryptogams; and he had the very unusual privilege of working in De Bary's laboratory at Strasbourg, where he associated intimately with other young men who were to continue the work of this great leader after his own untimely death.

Never overburdened by large numbers of half-interested students, Dr. Farlow communicated his own enthusiasm and industrious habits through long years to a limited number of men who have been counted for a generation among the leaders in American botany, and particularly in that branch of the science which De Bary's classical studies of fungous parasitism laid as the foundation on which the art of phytopathology has been reared of late, particularly in America, with much success and economic benefit.

Though familiar with ferns, and especially with the marine algæ of New England, of which he published an early monograph, Professor Farlow's interest always centered in the fungi, and the larger number of his publications have dealt with these plants.

He served his science particularly well in securing for permanent reference preservation the historic herbarium of Curtiss, one of the pioneers in American mycology, and that of Tuckerman, long the authority on American lichens; and since the death of Asa Gray, in 1887, he has been recognized at home and abroad as the foremost of American botanists.

Among his unpublished manuscripts is the completion of a compendious Bibliographic Index of North American Fungi, one volume of which was printed in 1905, and of which the remainder should be brought to publication promptly now that his work on it is done.

A keen critic, an encouraging teacher, a kindly and sympathetic friend, and a man of the broadest international interest, Professor Farlow is mourned by all who knew him.

[1] Memorial adopted by the Botanical Society of America.

SCIENTIFIC EVENTS

RESEARCH ON RUBBER CULTIVATION

A correspondent writes from Sumatra:

During the last week of August and the first week of September, 1919, Dr. J. J. van Hall, director of the Laboratory of Plant Diseases in Buitenzorg, Java, and Dr. R. D. Rands, botanist in the same laboratory; specially engaged on a study of the brown bast disease of the Hevea rubber, made a journey to Sumatra to study conditions there.

On September 2, 1919, a conference on brown bast disease was held at the A. V. R. O. S. (Algemeene-proefstation voor Rubber-Cultur, Oost-kust van Sumatra) Proefstation. This was attended by Acting Director F. C. van Heurn, of the A. V. R. O. S. Mr. J. C. Maas, and Dr. H. Heuser, also of the A. V. R. O. S., Dr. J. J. van Hall and Dr. R. D. Rands, both of the Laboratory of Plant Disease, Mr. Carl D. La Rue and Mr. P. E. Keuchenius, botanist and mycologist respectively, of the Holland-American Plantations Company, and Dr. J. G. Fol, director of the experiment station of the Cultur Maatschappij Amsterdam.

The cause of the disease was first discussed, Dr. Rands giving recent evidence secured by him pointing to a physiological origin. Mr. Carl D. La Rue stated that results obtained by Professor H. H. Bartlett and himself in 1918, and later by himself alone, indicated that the same bacterium was always present in bark affected with brown bark disease. Mr. Keuchenius stated that he also found bacteria to be constantly present in diseased tissue, and that he had secured positive results from inoculations with these bacteria.

Conditions favorable to attack by the disease were also discussed as well as methods of treat-

ment. All present agreed that the disease is the most serious one known to the rubber industry, that treatment alone was too expensive, and that methods of prevention should be discovered if possible.

Later at a special meeting an experiment was planned by Messrs. Rands, Maas, Keuchenius and La Rue to test more fully whether or not the disease may have a physiological cause. After visiting a number of rubber estates on the east coast of Sumatra and in Atjeh, Drs. van Hall and Rands returned to Java.

The first technical meeting of the personnel of the experiment stations for the rubber culture was held in Buitenzorg, Java, on November 1, 1919. Representatives of the Central Rubber Proefstation, the West-Java Proefstation, the Malang Proefstation, the Besoeki Proefstation, the Laboratorium voor Plantenziekten, and the research department of the Holland Plantations Company.

Among the subjects discussed were brown bast disease, mildew-diseases of leaves, borers, thinning out of trees on estates, and selection. The last topic is only now beginning to be a matter of concern to rubber planters, although experiment station workers have been interested in it for several years.

EXPERIMENT STATIONS OF THE BUREAU OF MINES

IN connection with the work of the Bureau of Mines, Department of the Interior, the bureau is now conducting eleven mining experiment stations, located in the various mining centers of the country, and bending their energies toward the special mining problems that are local to their part of the country. So great has been the demand for knowledge concerning the character of the work undertaken at these various mining stations and its general relation to the mining industry, the bureau has issued a bulletin describing the work of the stations. Dr. Van H. Manning, director of the bureau, sketches the work of the different stations as follows:

The station at Columbus, Ohio, situated at a clay-working center is employed mostly on ceramic problems. In this country there are about 4,000 firms manufacturing clay products, including brick, tile, sewer pipe, conduits, hollow blocks, architectural terra cotta, porcelain, earthenware, china and art pottery. The amount invested in these industries is approximately $375,000,000 and the value of the products exceeds $208,000,000 annually.

The station at Bartlesville, Okla., is investigating problems that arise in the proper utilization of oil and gas resources, such as elimination of waste of oil and natural gas, improvements in drilling and casing wells, prevention of water troubles at wells, and of waste in storing and refining petroleum, and the recovery of gasoline from natural gas.

What the Bureau of Mines has done for the great coal-mining industry, chiefly through investigations at the experiment station at Pittsburgh, Pa., has been published in numerous reports issued by the bureau. Some of the more important accomplishments have been the development and introduction of permissible explosives for use in gaseous mines, the training of thousands of coal miners in mine-rescue and first-aid work, and the conducting of combustion investigations, aimed at increased efficiency in the burning of coal and the effective utilization of our vast deposits of lignite and low-grade coal.

The Salt Lake City station has devised novel methods of treating certain low-grade and complex ores of lead and zinc. These methods show a large saving of metal over methods hitherto employed, and have made available ores that other methods could not treat profitably.

The Seattle station is busy with the beneficiation of the low-grade ores of the Northwest, and the mining and utilization of the coals of the Pacific states; the Tucson station is working on the beneficiation of low-grade copper ores; and the Berkeley station has shown how losses may be reduced at quicksilver plants and how methods at those plants can be improved.

In the conduct of these investigations the bureau seeks and is obtaining the cooperation of the mine operators. At more than a dozen mills in the west engineers from the stations are working directly with the mill men on various problems, and the results they already have obtained more than warrant the existence of the stations. Success in solving one problem may easily be worth millions to the country. Mining men are using these stations more and more freely as they realize that the government maintains these stations to help them, and that the difficulties of the operators, both large and small, will receive sympathetic consideration and such aid as the stations can give.

GRANTS FOR RESEARCH OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

AT the St. Louis meeting of the association, the council assigned the sum of $4,500 to be expended by the Committee on Grants for

Research during the year 1920. The members of the committe for the current year are: Henry Crew, chairman; W. B. Cannon, R. T. Chamberlin, G. N. Lewis, George T. Moore, G. H. Parker, Robert M. Yerkes, and Joel Stebbins, secretary.

The committee will hold a meeting in Washington in the month of April, when the distribution of the grants will be made. Applications for grants may be made under the general rules given below, which were adopted in 1917; but the committee especially invites suggestions from scientific men who may happen to know of cases where young or poorly supported investigators would be greatly helped by small grants.

1. Applications for grants may be made to the member of the committee representing the science in which the work falls or to the chairman or secretary of the committee. The committee will not depend upon applications, but will make inquiry as to the way in which research funds can be best expended to promote the advancement of science. In such inquiry the committee hopes to have the cooperation of scientific men and especially of the sectional committees of the association.

2. The committee will meet at the time of the annual meeting of the association or on the call of the chairman. Business may be transacted and grants may be made by correspondence. In such cases the rules of procedure formulated by the late Professor Pickering and printed in the issue of SCIENCE for May 23, 1913, will be followed.

3. Grants may be made to residents of any country, but preference will be given to residents of America.

4. Grants of sums of $500 or less are favored, but larger appropriations may be made. In some cases appropriations may be guaranteed for several years in advance.

5. Grants, as a rule, will be made for work which could not be done or would be very difficult to do without the grant. A grant will not ordinarily be made to defray living expenses.

6. The committee will not undertake to supervise in any way the work done by those who receive the grants. Unless otherwise provided, any apparatus or materials purchased will be the property of the individual receiving the grant.

7. No restriction is made as to publication, but the recipient of the grant should in the publication of his work acknowledge the aid given by the fund.

8. The recipient of the grant is expected to make to the secretary of the committee a report in December of each year while the work is in progress and a final report when the work is accomplished. Each report should be accompanied by a financial statement of expenditures, with vouchers for the larger items when these can be supplied without difficulty.

9. The purposes for which grants are made and the grounds for making them will be published.

JOEL STEBBINS,
Secretary

SCIENTIFIC NOTES AND NEWS

RICHARD C. MACLAURIN, president of the Massachusetts Institute of Technology since 1909, died from pneumonia in Boston on January 15. Dr. Maclaurin was born in Scotland in 1870. He was educated at the Universities of New Zealand and Cambridge, and was appointed professor of mathematics in the University of New Zealand in 1898. In 1907 he was appointed professor in mathematics and physics in Columbia University.

DR. JACQUES LOEB, of the Rockefeller Institute for Medical Research, was elected president of the American Society of Naturalists at the recent meeting held in Princeton.

PROFESSOR F. B. LOOMIS, of Amherst College, has been elected president of the Paleontological Society.

DR. PHOEBUS A. T. LEVENE, of the Rockefeller Institute for Medical Research, in New York, was elected associate member of the Société Royale des Sciences Médicales et Naturelles of Brussels, on December 1, 1919.

MR. J. H. JEANS, of Cambridge, formerly professor of mathematics in Princeton University, has been nominated as secretary of the Royal Society.

DR. PAUL SABATIER (Toulouse), and Dr. Pierre Paul Emile Roux (Paris), have been elected honorary members of the British Royal Institution.

THE Swedish Medical Association has awarded its jubilee prize this year to Dr.

Hans Gertz of the physiological laboratory of the Karolinska Institut for his work on the functions of the labyrinth. It was published in the *Nordisk Medicinskt Arkiv* in 1918.

THE president and fellows of Magdalen College of Oxford University on the express recommendation of the General Board of the Faculties decided to award a pension of £450 per annum to Professor Sydney Howard Vines, M.A., F.R.S., F.L.S., fellow of the college, and honorary fellow of Christ's College, Cambridge, who is resigning the Sherardian chair of botany with the fellowship on December 31 next, after a tenure of thirty-one years. This is the first occasion on which the new system of pensions for professors instituted by the college with the approval of the university has been brought into operation.

PROFESSOR EDGAR JAMES SWIFT, head of the department of psychology of Washington University, has been invited to give two lectures before the officers and students of the Post Graduate School of the United States Naval Academy at Annapolis. The subjects of these lectures are "Thinking and Acting" (February 14), and "The Psychology of Handling Men" (April 10).

UNIVERSITY AND EDUCATIONAL NEWS

AT the dinner of the alumni of the Massachusetts Institute of Technology, held in Cambridge on January 10, it was announced that the endowment fund of four million dollars had been obtained by the alumni, thus securing the gift of an equal sum from the hitherto anonymous "Mr. Smith." It was revealed that "Mr. Smith," who has now given eleven million dollars to the Massachusetts Institute of Technology, is Mr. George Eastman, president of the Eastman Kodak Company.

THE trustees of Oberlin College have granted increases of salaries for all in the service of the institution. Early in the fall the faculty of the college appointed a committee under the chairmanship of Professor C. G. Rogers to consider the salary needs of the members of the faculty. The report of the committee, approved by the faculty, was transmitted to the trustees, and findings calling for a fifty per cent. increase in the salaries of all teaching and administrative officers of the college, dating from September 1, 1919, were approved. This action adds about $175,000 to the annual budget of the college.

ANNOUNCEMENT has been made at the University of Pennsylvania of a gift of $50,000 from the estate of William C. Goodell for the establishment of a chair of gynecology in the medical school. The trustees have adopted a resolution providing that as far as possible rooms and facilities for the carrying on of research work be extended to emeritus professors in all departments.

THE pathological buildings of the Johns Hopkins Hospital group, the professional workshop of Dr. William H. Welch, was wrecked by fire, January 12. It is said that none of the valuable specimens was lost, nor were any of the records of research work damaged.

PROFESSOR A. P. COLEMAN, geology, has been elected dean of the faculty of arts of the University of Toronto. Professor J. Playfair McMurrich, anatomy, has been elected chairman of the board of graduate studies, which corresponds with the graduate faculty in many universities.

DR. HAROLD PRINGLE, lecturer on histology and assistant in the department of physiology in the University of Edinburgh, has been appointed professor of physiology in Trinity College, Dublin, in the room of the late Sir Henry Thompson.

DR. F. W. KEEBLE, assistant-secretary of the British Board of Agriculture, has been elected to the Sherardian professorship of botany of Oxford University in succession to Professor S. H. Vines.

DISCUSSION AND CORRESPONDENCE

THE POLYDOGMATA OF THE PHYSICIST

THE mind of the physicist may be said to be somewhat in confusion. But there is no reason to hope that it ever will enjoy the

logical perfection of a consistent set of theories. He constructs the electromagnetic theory of light and must needs adhere to it on many occasions, yet with full knowledge that it can not be correct. He rejoices in the existence of the universal constant, h, and the appearance of the quantum, $h\nu$, in resonance and ionization potentials, in photoelectric X-ray phenomena, and in the theory of heat radiation, yet he can not be reconciled to the existence of the quantum in the phenomenon of the passage of light through a vacuum. He builds an atomic structure which will not only provide a clear picture, but will also furnish quantitative results in striking agreement with experiment; and yet he must, in his building, reject certain principles which elsewhere he adopts without hesitancy. He rejoices in the achievement of the general theory of relativity, which, unless proved untenable, gives a logical consistency at present—and probably for many, many years, unattainable by other means; yet in his constructive thinking he sometimes uses the ether which the general theory of relativity ignores, and he lives in his old Euclidean world which the present developments from this theory deny.

In short, the physicist can not be consistent. Moreover, he can not progress unless this inconsistency is a stimulus and not an annoyance. He must live as if in several compartments, enjoying in each one the consistency possible therein, and being not distressed but rather interested and invigorated by the failure to unite these compartments into one consistent whole. If he "believes," he must be inconsistent. If he progresses, he must adopt a set of dogmas in the small compartment in his immediate problem. If he follows with full sympathy modern progress in physics, he must have not one, but many dogmas, and these not wholly consistent with one another.

I refer not merely to the multiple-theory method of attack upon a problem, for the dogmas are not even altogether similar in kind, but more especially to the ability to appreciate thoroughly not only "constructive theories," but also "theories of principle" (quoting from Einstein) It is not merely the approach from a different viewpoint in the same universe, but it is the ability to live in more than one universe.

All of this may be obvious, but yet, in point of fact, now and again there appears evidence that even physicists of note are pained by this rôle. They seem to resist by objections which do not aid in the extension of these compartments, or by a rebellion against the obviously advantageous policy of polydogmata.

G. W. Stewart

State University of Iowa

TOTEM POLES FOR MUSEUMS

Fifty years ago some of the best totem poles of the Haida Indians of Queen Charlotte Islands cost the Indians several thousand dollars each. To-day many of these may be purchased for a dollar and a half or two dollars a foot. That is, an average totem pole can be purchased, crated and put aboard a steamer at Masset for about one hundred dollars.

Many of the Haida totem poles have disappeared. A few have been taken to museums where they are preserved; some have been burned; many have decayed. Several, seen during the past summer, at Yan opposite Masset, have recently been blown over by the wind. In a few years all will have disappeared unless means are taken to save specimens of this art for the future. However the other tribes having totem poles may feel at this date, the Haidas have come to the point of neglecting the poles and being willing to sell them. They are owned by families, and negotiations as with an estate are necessary for properly obtaining them.

This North Pacific art is one of the treasures of Canada and the United States. Examples of it should be preserved in each large city of the continent. It may not be generally known how easily this can be done.

In the summer the Haidas of Masset are busy fishing. In the spring they have less to do and some are in need of money. Mr. Alfred Adams or Mr. Henry Edensaw are trust-

worthy Haidas of Masset, B. C., who are capable of corresponding and executing the purchase of a pole or poles, and of engaging other help and superintending the lowering and creating of poles, their transportation across the inlet from Yan to the wharf at Masset and their shipment to destination. The poles are very heavy and the cost of handling will be perhaps equal to the price of the poles. They are soft and their own weight will crush parts of the carvings unless they are properly crated. Some of the poles 50 to 60 feet in length may have to be cut in sections for shipment.

Here is an opportunity. Examples of this unique art now going to decay may be rescued, loaded and started on their way to safe-keeping in our museums at the rate of about one hundred dollars per specimen.

HARLAN I. SMITH
GEOLOGICAL SURVEY,
OTTAWA, CANADA

TO KILL CATS FOR LABORATORY USE

A QUICK and humane method of killing a cat or other small mammal in the laboratory is to put the animal under an open topped bell jar, *i. e.*, a bell jar which has a small bottle-like neck at the top through which there is an opening. This mouth should be comparatively small, not over a half inch in diameter, and the neck should be at least an inch long. After the animal has been placed under the bell jar, a very small quantity of ether or chloroform is poured through the opening in the top, and it is then corked up. The liquid strikes the sides of the neck and immediately runs down in a thin film over the inner surface of the bell jar and evaporates into the chamber in two or three seconds. The enclosed animal shows its effects almost immediately, and dies in a very short time.

While it is not necessary, it is better to seal up the base of the bell jar because occasionally the animal falls down after it becomes unconscious, and its head comes in close proximity to the crack between the jar and the object on which it is placed, and it thus obtains sufficient air to delay its death. This can be prevented by wrapping a damp towel around the base so as to exclude the air. By placing the bell jar on a glass plate and sealing with vaseline, an airtight chamber can be made, but the advantage thus gained does not make up for the care necessary in order to avoid getting one's clothing in contact with the greased surfaces.

HORACE GUNTHORP
WASHBURN COLLEGE,
TOPEKA, KANS.

ANTS AND SCIENTISTS

TO THE EDITOR OF SCIENCE: As a result of watching a colony of ants and attending a scientific meeting on the afternoon and evening of the same day, it seemed to me the two teeming hordes of excited workers—the insects and the scientists—had some queer traits in common, as:

1. How they work in ranks and cohorts, mutually attracted by some exciting discovery that a wandering member has stumbled upon, and that awakens the most astounding and intense interest.

2. How they immediately set to work to pull opposite ways, fight valiantly over their treasure, and heroically keep it up after they have amputated some of each others' legs and other appendages.

3. How they take up one thing, drag it about for a time, and then drop it for some other thing.

4. How they often expend enormous labor on something that isn't worth a darn; and here Mark Twain's story of the two ants and the grasshopper leg came to mind.

5. How their splendid industry is generally circular in direction; so that after long struggle, they get the thing back to the exact spot from which it started.

6. How they firmly believe that "they are the people" and refuse to admit or bother over bigger intelligences that are their interested observers and that can and sometimes do sweep them and their hills and runways and stores into oblivion.

7. How, measured by final results, they are nevertheless a wonderful body of workers;

and in tireless energy, patience and talent, stand out preeminent in their respective groups.

Albert Mann

QUOTATIONS

THE BRITISH NATURAL HISTORY MUSEUM

We learn that there are at present vacancies in the entomological, zoological and geological departments of the Natural History Museum which have been open for several months, and that more vacancies are expected in the immediate future. The museum is one of the great national instruments for the collection, classification, and preservation of specimens of the animal and plants, the rocks and minerals, of the world. For the adequate performance of its duties, it must have a full staff of able and devoted specialists. It should require no defense on utilitarian grounds, for the advancement of natural knowledge of the kind to which it is devoted is recognized as a privilege by every civilized state. But there are plenty of utilitarian arguments. Take entomology alone: the number of living species of insects is estimated at over 2,000,000. The preserver of insect life on human life is continuous. As household pests, as carriers of disease, as enemies of stores or crops, they are every day being found to have an unexpected economic importance. It is to the experts and the collections of the Natural History Museum that we have to turn for the requisite information, and unless the museum has an adequate staff we turn in vain. The difficulty in filling posts with suitable men is partly financial. The present rate of pay for assistants in the second class is from £150 to £300, and in the first class from £300 to £500 a year, with a temporary war bonus. These salaries—the "despair" of Professor Stanley Gardiner, whose cogent letter we publish in another column—are no longer sufficient to attract or to retain men of the right attainments, unless they happen to have private means. The smallness of the staff and its inevitable division into water-tight compartments makes promotion slow and capricious. These disadvantages are increased by an antique privilege of the principal trustee, who nominates candidates for vacancies instead of advertising for them. It has frequently happened in the past that middle-aged mediocrities have been brought in and placed over the heads of the existing staff because of their acquaintance with a group in which some of the trustees are interested. The fact is that the mode of governance of the Natural History Museum is medieval. It should be separated from Bloomsbury and placed under a body of trustees selected not because they make a hobby of collecting bugs or butterflies, but because they have a wide knowledge of the scientific purposes which it is the business of the museum to subserve.—*The London Times.*

SCIENTIFIC BOOKS

Geodesy, including Astronomic Observations, Gravity Measurements and Method of Least Squares. By George L. Hosmer. John Wiley and Sons. First edition, 1919, 377 pages, 6 × 9, 115 cuts.

This book is especially to be commended for the skill shown in the selection of illustrations, both photographs and drawings, and for the excellence of arrangement and printing of the text and tabular matter. These things contribute substantially to the satisfaction and comfort of the user.

Still more is the book to be commended for its positive qualities, which make it a distinct and valuable addition to that part of the literature of geodesy which serves to carry information and understanding from the extreme specialists who are developing the methods and extending the knowledge in these fields, to the students and the practising engineers who desire to get a well-balanced view of the whole field of geodesy quickly. The old well-known matters are restated well in effective grouping. The ideas, formulæ and tables most needed by the student and the practising engineer are selected from the great mass of available material with rare skill. The recent developments in geodesy are shown in true perspective with respect to old things, to a quite unusual extent for a text-book.

Among the comparative recent developments in geodesy that are especially well stated in the book are (1) the importance of determining the relative strength of different proposed chains of triangulation as fixed by the geometrical relations, and the methods for quickly doing so; (2) the relation between the average length of the lines in a triangulation and the rapidity, economy, and accuracy of that triangulation and its convenience to the user; (3) the advantages of the light and rapidly built towers such as are now used in the Coast and Geodetic Survey; (4) the advantages of the transit micrometer on portable instruments for determining time accurately; (5) the application of the interferometer to determination of the flexure of the support of a pendulum used to determine the relative values of gravity at different points. These things are stated forcefully and with good judgment as to their relation to older ideas and methods.

Though he has looked carefully for errors of omission, the reviewer, who has a background of experience which naturally tends to make him keenly critical, finds only three that are, in his opinion, important.

1. On its best direction theodolites the Coast and Geodetic Survey uses two sets of double parallel lines in the micrometer microscopes with which the horizontal circle is read, the two sets being so placed that the observer moves the micrometer screw only one turn between a forward and the corresponding backward reading, instead of five turns. This is a time-saving convenience which also increases the accuracy, and surely should have been mentioned in the book.

2. The necessity of tracing back the adopted field length of a base measuring tape to the standard meter and the methods of doing so are inadequately treated in the book. The developments of the past twenty years have made it clear that one must concentrate much more keenly on this part of the work than the book indicates.

3. The area method of computing the figure of the earth from geodetic and astronomic observations is barely referred to on page 204 without explanation. In view of the fact that this method gives a much higher degree of accuracy from the same observations than the traditional arc method, it certainly deserves a page of general exposition in the book, even if it is possibly too difficult for the student to grasp in full. The student and the engineer should know that the more accurate method exists, should know its general character, and in a general way why it is more accurate than the arc method.

The author of the book has shown such ability to see with the eye of an expert, and to exercise the judgment of a practicing geodetic engineer, that one may confidently expect that even these three omissions will not occur in a second edition.

JOHN F. HAYFORD

SPECIAL ARTICLES

CONCERNING APPLICATION OF THE PROBABLE ERROR IN CASES OF EXTREMELY ASYMMETRICAL FREQUENCY CURVES

IN a study of the fecal pollution of shellfish, Dr. James Johnstone[1] raises an important question: that of determining the most probable value of a measure from a series whose frequency distribution is highly asymmetrical. In such instances it is evident, although prevailing practise contradicts the statement, that it is illegitimate to apply the probable error in the usual manner. For such application presupposes a symmetrical (Gaussian) distribution, and, since a wide range of biological measurements is characterized by an asymmetrical distribution, the matter merits consideration.

Dr. Johnstone lists the following counts of colonies of bacteria growing on twenty plates, each having been incubated a standard length of time after being inoculated with 1 c.c. of an emulsion, in 250 c.c. of water, of five muscles collected at random from the polluted area: 7, 24, 40, 15, 22, 20, 17, 9, 16, 29, 7, 9, 10, 26, 15, 11, 21, 17, 10, and 41. Dr. Johnstone assumes each count to be an estimate of the number of bacteria per c.c. of the emul-

[1] "The Probable Error of a Bacteriological Analysis," Rept. Lanc. Sea-Fish. Lab., 1919, No. XXVII., p. 64–85.

sion, the variation between the counts being attributed to errors in sampling. He then raises the question as to the most probable number of bacteria present, and, after pointing out that, according to custom, the arithmetic mean of the counts (18.3) would be regarded as the most probable number, proves this to be untrue by showing the frequency distribution to be highly asymmetrical, as follows:

Counts	Frequency
6–10	6
11–15	3
16–20	4
21–25	3
26–30	2
31–35	0
36–40	1
41–45	1

Although Dr. Johnstone discusses this distribution, and, by employing Galton's graphical method, determines certain constants, he fails to answer the question he raises.

In cases of this kind it seems as though the simplest procedure is to find some function of the measurements whose frequency distribution is Gaussian, and apply the probable error to that function. The reason is that an asymmetrical distribution implies that some influence other than "chance" is operative, and substitution of a function whose distribution is Gaussian enables their separation. In the particular case at hand, and it is typical of many within the province of biology, this function is the logarithm. This is easily demonstrated by grouping the logarithms of the counts with respect to a deviation of ± 0.1 from their mean ($=1.2046$) as follows:

Logarithm	Frequency
0.505–0.704	0
0.705–0.904	2
0.905–1.104	5
1.105–1.304	6
1.305–1.504	5
1.505–1.704	2
1.705–1.904	0

The arithmetic mean of the logarithms (1.2046) is the logarithm of the geometric mean of the counts ($=16.02$), the geometric mean, by definition, being the twentieth root of the product of the twenty counts. Accordingly, the Gaussian distribution of the logarithms shows that the counts cluster in approximately constant ratio about their geometric mean, or, to express it otherwise, that variations in the count are compensatory in the geometric mean. This signifies that variation in the count is not primarily attributable to errors in sampling and that each count is not an estimate of the number of bacteria present per c.c. in a homogeneous emulsion, but rather that conditions favoring the propagation of bacteria fluctuated in an "accidental" way either during the period in which the twenty samples were removed from the emulsion, or from place to place within the emulsion, or both. Whether or not this interpretation be correct, the logarithmic frequency distribution demonstrates that something of like nature occurred. In any case the most probable number of bacteria per c.c. corresponding to the most typical condition of the emulsion is the geometric mean of the counts (16.02); and, in the same sense, $250 \times 16.02 = 4{,}005$ is, of course, the most probable number of bacteria in the whole emulsion.

The reliability of this estimate may be approximated by applying the probable error to the logarithms. The standard deviation of the logarithms, σ, is 0.224, the probable error, or, better, the "probable departure" from the logarithm of a single count is $0.6745\ \sigma = \pm 0.1511$ and the probable departure from the logarithmic mean is $0.1511/\sqrt{20} = \pm 0.0337$. It follows from tabulated values of the probability integral that, had the entire 250 c.c. been examined, it is as likely that the logarithmic mean would have been within 1.2046 ± 0.0337 as that it would have been outside these limits, while the odds are about 4.6 to 1 that it would have been within $1.2046 \pm 2(0.0337)$, about 22 to 1 that it would have been within $1.2046 \pm 3(0.0337)$, and nearly 142 to 1 that it would have been within $1.2046 \pm 4(0.0337)$. The numbers corresponding to these logarithms are the limit-

ing values of the estimated number of bacteria per c.c.; that is, the odds are even that this number lay between 14.82 and 17.31, about 4.6 to 1 that it lay between 13.72 and 18.72, about 22 to 1 that it lay between 12.69 and 20.22, and nearly 142 to 1 that it lay between 11.74 and 21.86.

This, I believe, answers Dr. Johnstone's question in so far as the small series of counts permit. The problem is typical of many that have not received due consideration by either biologist or statistician; and conclusions departing widely from the truth are continually being reached through failure to apply any criterion of reliability on the one hand, and through an erroneous application of the probable error on the other hand. It is hoped this brief presentation will stimulate discussion.

ELLIS L. MICHAEL

SCRIPPS INSTITUTION,
LA JOLLA

THE AMERICAN MATHEMATICAL SOCIETY

THE twenty-sixth annual meeting of the society was held at Columbia University on Tuesday and Wednesday, December 30–31, with the usual morning and afternoon sessions on each day. The attendance included 96 members. President Frank Morley occupied the chair, being relieved at the last session by Professor J. L. Coolidge. The following new members were elected: Dr. H. E. Bray, Rice Institute; Professor I. L. Miller, Carthage College; Dr. Helen B. Owens, Cornell University; Professor E. W. Pehrson, University of Utah. Ten applications for membership were received.

At the annual election the following officers and other members of the council were chosen: vice-presidents, C. N. Haskins and R. G. D. Richardson; secretary, F. N. Cole; treasurer, J. H. Tanner; librarian, D. E. Smith; committee on publication, F. N. Cole, Virgil Snyder, and J. W. Young; members of the council to serve until December, 1922, T. H. Hildebrandt, Edward Kasner, W. A. Manning, H. H. Mitchell.

The total membership of the society is now 733, including 80 life members. The total attendance of members at all meetings, including sectional meetings, during the past year was 393; the number of papers read was 187. The number of members attending at least one meeting during the year was 252. At the annual election 156 votes were cast. The treasurer's report shows a balance of $10,692.23, including the life membership fund of $7,168.87. Sales of the society's publications during the year amounted to $1,811.52. The library now contains 5,690 volumes, excluding some 500 unbound dissertations.

It was decided to proceed with the incorporation of the society under the general law of the state of New York. A committee was appointed to consider plans for the organization and administration of the society after the retirement of the present secretary and librarian from their offices at the close of the present year. A committee was also appointed to consider the formation of an international union of mathematicians. The committee on mathematical requirements presented a report, which was laid over for consideration at the February meeting.

The following resolutions, introduced by Professor R. C. Archibald as chairman of the committee on bibliography, were adopted by the council:

> The council regards the preparation and publication, in America, of a dictionary of mathematical terms as not only most desirable but also entirely feasible, provided that financial aid for the preparation of the manuscript can be secured.
>
> Impressed with possibilities for the more extensive development of pure and applied mathematics in America, and with the importance of such development to the nation, the Council records its conviction that there are undertakings whose active consideration would be highly desirable if adequate financial assistance might be regarded as available. Among such undertakings are: 1. The preparation and publication by societies or individuals of surveys, introductory monographs, translations, memoirs, and treatises, in important fields, including the history of mathematics. 2. The organization of research fellowships. 3. The preparation and publication of an encyclopædia of mathematics in English. 4. The preparation and publication of an annual critical survey, in English, of the mathematical literature of the world. 5. The preparation and publication of a biographical and bibliographical dictionary of mathematicians.

The meeting of the society immediately preceded that of the Mathematical Association of America on January 1–2. A very pleasant occasion was the joint dinner of the two organizations on New Year's eve with an attendance of 114 members and friends.

The following papers were read at the annual meeting:

The sum of the face angles of a polyhedron in space of n dimensions: H. F. MACNEISH.

A connected set of points which contains no continuous arc: G. A. Pfeiffer.

Fundamental types of groups of relations of an infinite field: C. J. Keyser.

The theorem of Thomson and Tait and its converse in space of n dimensions: Joseph Lipka.

Poncelet polygons in higher space: A. A. Bennett.

Continuous matrices, algebraic correspondences, and closure: A. A. Bennett.

Concerning points of inflection on a rational plane quartic: L. A. Howland.

Geodesics motion on a surface of negative curvature: H. C. M. Morse.

The geometry of Hermitian forms: J. L. Coolidge.

Rotations in space of even dimensions: H. B. Phillips and C. L. E. Moore.

Note on geometric products: C. L. E. Moore and H. B. Phillips.

A memoir upon formal invariancy with regard to binary modular transformations: O. E. Glenn.

The invariant problem of the relativity transformations of Lorentz appertaining to the mutual attraction of two material points: O. E. Glenn. (Preliminary report.)

The mean of a functional of arbitrary elements: Norbert Wiener.

Bilinear operations generating all operations rational in the domain Ω: Norbert Wiener.

Fréchet's calcul fonctionnel and analysis situs: Norbert Wiener.

A set of postulates for fields: Norbert Wiener.

On the location of the roots of the jacobian of two binary forms, and of the derivative of a rational function: J. L. Walsh.

On the proof of Cauchy's integral formula by means of Green's formula: J. L. Walsh.

On the order of magnitude of the coefficients in trigonometric interpolation: Dunham Jackson.

A problem of electrical engineering: P. L. Alger.

Properties of the solutions of certain functional differential equations: W. B. Fite.

Determination of the pairs of ordered real points representing a complex point: W. C. Graustein.

Sheffer's set of five postulates for Boolean algebras in terms of the operation "rejection" made completely independent: J. S. Taylor.

$_nE_x$, the magic wand of actuarial theory: C. H. Forsyth.

A formula for determining the mode of a frequency distribution: C. H. Forsyth.

Asymptotic orbits near the equilateral triangle equilibrium points in the problem of three finite bodies: Daniel Buchanan.

The definition of birational transformations by means of differential equations: C. L. Bouton.

Area-preserving, parallel maps in relation to translation surfaces: W. C. Graustein.

Note on linear differential equations of the fourth order whose solutions satisfy a homogeneous quadratic identity: C. N. Reynolds, Jr.

A practical problem of aerodynamics and thermodynamics: J. E. Rowe.

A property of permutation groups analogous to multiple transitivity: W. B. Carver and Mrs. E. F. King.

Some pseudo-finiteness theorems in the general theory of modular covariants: Olive C. Hazlett.

Note on the rectifiability of a twisted cubic: Mary F. Curtis.

The representation of fractions of periods on algebraic curves by means of virtual point sets: Teresa Cohen.

Necessary and sufficient conditions that a linear transformation be completely continuous: C. A. Fischer.

On the structure of finite continuous groups with a single exceptional infinitesimal transformation: S. D. Zeldin.

On the location of the roots of the derivative of a polynomial: J. L. Walsh.

Abstracts of the papers will appear in the March issue of the society's *Bulletin.*

The thirteenth western meeting of the society, being a joint meeting of the Chicago and Southwestern Sections, was held at St. Louis on the same days as the meeting in New York. The next regular meeting of the society will be held in New York on February 28.

F. N. Cole,
Secretary

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